

that the advisory group used evidence from trials in shaping the delivery of services.

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## Randomised controlled trials of treatment are needed

EDITOR,—The current interest in low back pain<sup>1</sup> is to be welcomed, particularly in the light of the doubling of the rate of male sickness and payments of invalidity benefit for spinal disorders in the 10 years to 1991–2.<sup>2</sup> The rate of payment of benefit, however, is an unreliable measure of the burden of back pain in the community because it principally measures long term disability, not pain and disability in those who can continue to work or who are economically inactive.

Two key points in the Clinical Standards Advisory Group's recommendations for a revolution in the treatment of low back pain are that physical treatment should be focused on the early stages of the condition to prevent it becoming chronic and that a new specialist back pain rehabilitation service should be developed for patients who have had back pain for over six weeks.<sup>2</sup> The clear aim is to reduce long term absence due to sickness. With the increasing emphasis on evidence based medicine, we should carefully consider whether early physical treatment after initial treatment in general practice will reduce the burden of long term disability from low back pain before we start major changes. The advisory group's report does not convincingly show this.

Two randomised trials with multiple outcome measures, published after the advisory group's report, compared conventional management in primary care with and without physiotherapy exercises for acute simple back pain.<sup>3,4</sup> One failed to show a difference in outcome after one year, while the other found that conventional management in primary care resulted in a significantly better outcome after 12 weeks. Another trial showed, as its only outcome measure, a 50% reduction in long term sickness absence after a clinical and radiological assessment with advice to maintain activity compared with conventional treatment.<sup>5</sup>

Before there is a revolution in the treatment of acute back pain we should obtain proof from randomised controlled trials that true reductions in the level of pain or disability suffered, or both, can be achieved. Any future change in the rate of payments of benefit for spinal disorders cannot be relied on as a single measure of success or failure in treating back pain.

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## Complications of juvenile chronic arthritis

### Frequency of eye screening should suit individual patients

EDITOR,—A potential source of confusion in T R Southwood's article on arthritis in children is the question of screening for chronic anterior uveitis.<sup>1</sup> Southwood advises screening every three to six months except in low risk groups. A report published recently by a joint working party of the Royal College of Ophthalmologists and the British Paediatric Association recommends that this should be so for five years from the onset of juvenile chronic arthritis for high and medium risk groups, with subsequent screening annually for 10 years after the onset of juvenile chronic arthritis, or until the age of 12, whichever is shorter.<sup>2</sup> The report also clearly defines the different risk groups for uveitis: high risk—early onset (age less than 6 years), pauciarticular disease, positive for antinuclear antibodies; medium risk—polyarticular disease and positive for antinuclear antibodies, or pauciarticular disease and negative for antinuclear antibodies; and low risk—systemic juvenile chronic arthritis, juvenile chronic arthritis associated with HLA-B27, or disease starting after the age of 11 years. The report also expands on the "screening every three to six months" for the respective groups.

I have often found that children are re-referred from colleagues sooner than necessary for screening or that general practitioners have become concerned because their patients have not been seen in the eye clinic for almost a year, when this may be appropriate. In addition, the end point of screening does not seem to be widely known. Obviously, clinic visits that are more frequent than recommended interrupt schooling more and add further disruption to the child's family.

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- 1 Southwood TR. Arthritis in children. *BMJ* 1995;310:728–32. (18 March.)
- 2 Royal College of Ophthalmologists and the British Paediatric Association. *Report of a joint working party: ophthalmic services for children*. London: RCO, BPA, 1994.

### Surgical treatment of septic arthritis is superior

EDITOR,—I am concerned by T R Southwood's suggestions for the management of childhood septic arthritis.<sup>1</sup> Southwood suggests that affected joints should be aspirated for diagnostic purposes before treatment with antibiotics is given. This practice seems to be common among rheumatologists and results in the late referral to orthopaedic surgeons of patients with septic arthritis. At the time of referral the joints of these patients are often severely damaged, and this may result in lifelong disability. Early surgical intervention can prevent many of these poor results.

Southwood should at least have mentioned arthrotomy and joint washout, which is the safest way of treating septic arthritis to prevent joint destruction.<sup>2</sup> In certain joints arthroscopic washout is an alternative, but this may be associated with a higher recurrence rate. Repeated joint aspiration for early infection also has its advocates,<sup>3</sup> but if rapid resolution does not occur arthrotomy and washout is advocated. Clearly this approach cannot be used in late infection as loculation and solid debris which cannot be broken down or cleared with a needle will be present in the joint.

These patients should thus be referred early for an orthopaedic opinion as they require early surgical intervention to prevent joint damage and resultant disability and may also need prolonged

orthopaedic surveillance and treatment in the future if treatment fails.

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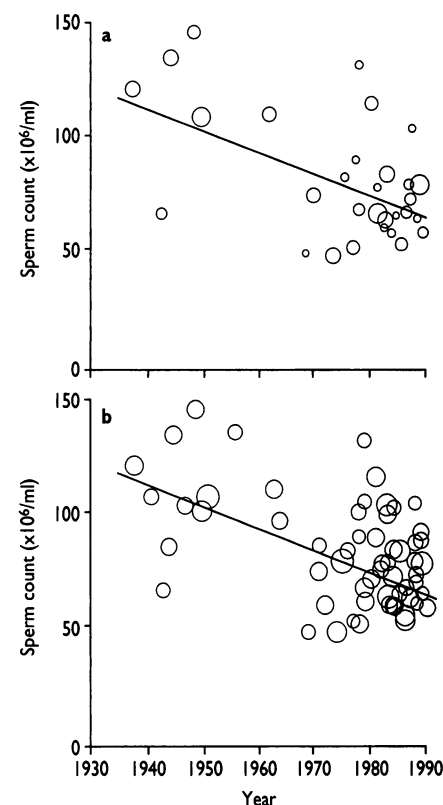
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## Evidence for decreasing quality of sperm

### Presentation of data on sperm concentration was flawed

EDITOR,—Few studies have attracted as much attention as that by Carlsen *et al* on the decline in human sperm concentrations over the past 50 years.<sup>1</sup> Few studies, however, have caused so much controversy as well.<sup>2,4</sup> Carlsen *et al* analysed 61 publications from 1938 to 1990 in which sperm samples from presumably normal men were investigated. In a table the authors gave, for each publication, the number of samples in the study, and the men's fertility status. Linear regression analysis showed a highly significant trend towards lower sperm concentrations. In their figure 1 the authors stated that they gave 61 datasets as circles, with year and sperm count as coordinates and the circles' area representing the logarithm of the number of subjects in each study (fig 1a). Brake and Krause<sup>2</sup> and Olsen *et al*<sup>3</sup> criticised Carlsen *et al*'s paper for various statistical reasons as well as for methodological reasons—for example, that the abstinence times were different. Likewise, Bromwich *et al* pointed out that a change in the "normal" values over time may have been a possible explanation for the observed trend.<sup>4</sup>



Original figure 1 as given in paper by Carlsen *et al* (a) and redrawn figure (b) based on data given in their table (for comparison the regression line is also included, although an independent statistical analysis was not performed)

When comparing the recent report by Olsen *et al*<sup>3</sup> with the original paper by Carlsen *et al* I noticed differences in the graphs. Figure 1 of the original paper contains data for only 31 of the 61 publications listed in the table: 30 data points are missing. Furthermore, the difference in the circles' areas is greater than expected: the ratio of the maximum to the minimum number of subjects in the studies, expressed as a logarithm, is 4.3, but the difference in the areas of the circles is far larger (fig 1a). Using the data given in the original paper's table, I redrew the figure (fig 1b). The overall impression is quite different.

I wonder about the reason(s) for these mistakes. As this paper had a considerable impact not only in the scientific community but also in the lay press, it is difficult to comprehend why these severe errors have been overlooked both before and after publication.

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- 1 Carlsen E, Giwercman A, Keiding N, Skakkebaek NE. Evidence for decreasing quality of semen during past 50 years. *BMJ* 1992;305:609-13.
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- 3 Olsen GW, Bodner KM, Ramlow JM, Ross CE, Lipshultz LJ. Have sperm counts been reduced 50 percent in 50 years? A statistical model revisited. *Fertil Steril* 1995;63:887-93.
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## Authors' reply

EDITOR,—As Alexander Lerchl points out, figure 1 of our overview indicating that sperm concentrations have decreased during the past 50 years is deficient. During the final preparation for publication, for reasons that we cannot trace, some of the points were omitted. The regression analysis in the paper is unaffected by this: the new regression line (weighted by the number of subjects) had a slope of  $-0.934 \times 10^6/\text{ml}$  per year (SE 0.157;  $P < 0.0001$ ), and that line was correctly included in our original figure. A better impression of the regression analysis is provided by the figure in this letter, in which the areas of the circles are proportional to the number of subjects in each publication. There is no reason for Lerchl's scepticism.

Lerchl quotes several criticisms of our paper but omits our detailed and specific responses as well as the subsequently published empirical evidence, which points in the same direction as our paper.

Specifically, Lerchl quotes Brake and Krause, who, on the basis of our data, claimed that sperm concentration had significantly increased since 1970. In fact, Brake and Krause made a mistake in their calculation: the increase they quoted is non-significant ( $P=0.36$ ). Lerchl quotes Bromwich *et al*, who offered a speculative, elementary statistical argument with no empirical basis or verification. Lerchl fails to quote our earlier detailed

comments on this theoretical exercise.<sup>1,2</sup> Lerchl finally quotes the recent report by Olsen *et al*, who also did not add new empirical evidence: they performed various unsurprising reanalyses of our data, all of which agreed about a significant decline in sperm concentration. We have submitted detailed comments on these reanalyses elsewhere.

Lerchl omits to refer to the additional empirical evidence that has been published. Auger *et al* (who were originally motivated by serious scepticism about our original report) studied 1351 healthy men volunteering to donate sperm in one clinic in Paris between 1973 and 1992.<sup>3</sup> Carefully separating age effects from cohort effects (year of birth), they documented a highly significant decrease in sperm count of 2.1% per year (from  $89 \times 10^6/\text{ml}$  in 1973 to  $60 \times 10^6/\text{ml}$  in 1992) and concomitant decreases in the percentages of mobile and normal spermatozoa. Three additional, shorter reports have been published, also based on data from one clinic and all with similar conclusions.

In a recent international effort the temporal trends in semen quality were viewed in a broader context.<sup>4</sup> There have been similar temporal increases in the incidence of testicular cancer and frequently of hypospadias and cryptorchidism, and geographical covariation of several of these symptoms as well as male breast cancer has been documented. In our view it would be irresponsible to disregard this evidence, even if the link to possible determinants is far from definitively established.

Although Lerchl points out a (qualitatively unimportant) deficiency in figure 1 of our paper, we hope that this will not delay a dedicated, wide ranging research effort to clarify these issues.

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- 1 Keiding N, Giwercman A, Carlsen E, Skakkebaek NE. Importance of empirical evidence. *BMJ* 1994;309:22. [Commentary on: Bromwich P, Cohen J, Stewart I, Walker A. Decline in sperm counts: an artefact of changed reference range of "normal"? *BMJ* 1994;309:119-22.]
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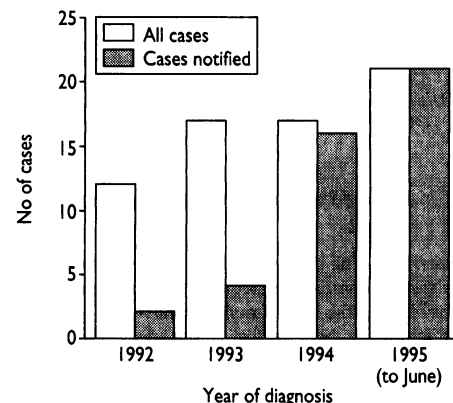
## Reasons for increased incidence of tuberculosis

### Audit suggests that undernotification is common

EDITOR,—In her editorial Janet H Darbyshire suggests that undernotification of tuberculosis, particularly in association with HIV infection, is still common.<sup>1</sup> If sufficiently widespread, undernotification could result in underestimation of the incidence of tuberculosis, particularly in patients coinfected with HIV, with considerable public health implications. We recently audited notification of tuberculosis in patients known to be infected with HIV who were attending our hospital.

A database on all patients with mycobacterial infection was established by searching microbiology, histopathology, and clinical computerised records systems. Case notes were then examined for all patients. Patients were considered to have

tuberculosis on the basis of a positive result of culture of a specimen from any site or either histological or radiographic changes compatible with tuberculosis and a response to standard antituberculous treatment. This database was then cross referenced with a record of notifications for the whole hospital. The figure shows the results.



Number of cases of tuberculosis diagnosed and notified in patients with HIV infection, 1992-5

Tuberculosis was considerably undernotified in 1992. The reasons for this were not clear from this audit, but the appointment of a clinical nurse specialist who had specific responsibility for notification of, and contact tracing in, cases of tuberculosis and HIV infection led to a substantial improvement in the rate of notification. This suggests that clinicians' concerns about patient confidentiality were not the prime reason for undernotification. In addition, a considerable increase in the numbers of cases of tuberculosis in patients also infected with HIV has been seen this year. Although the number of notifications of tuberculosis from our hospital has risen, from 99 in 1992 to 60 in the first six months of this year, the proportion of patients with HIV infection has increased from 17% to 32% over the same period. This seems to be due to increased screening for HIV infection in patients with tuberculosis. We have thus shown that although undernotification of tuberculosis in patients with HIV infection occurs, improved notification may also lead to increased recognition of coinfection with HIV.

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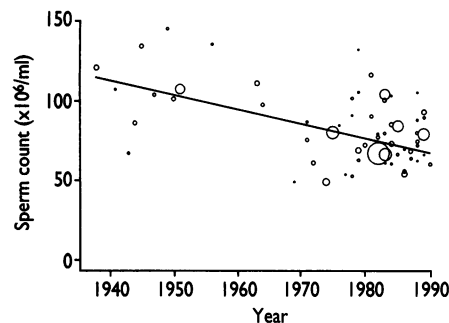
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- 1 Derbyshire JH. Tuberculosis: old reasons for a new increase. *BMJ* 1995;310:954-5. (15 April.)

### Large immigrant population may have confounded study

EDITOR,—N Bhatti and colleagues present an interesting analysis of changing rates of notification of tuberculosis based on national notifications and local data from Hackney.<sup>1</sup> The findings are interpreted as suggesting that the national increase is largely due to socioeconomic factors that have affected the white population and established ethnic minority communities to a similar extent. The authors suggest that recent immigration has made only a small contribution to this increase. The study's findings do not justify these conclusions.

As quoted in the paper, markers of socio-



Linear regression of mean sperm density reported in 61 publications (represented by circles whose area is proportional to number of subjects in study), each weighted according to number of subjects, 1938-90